

interest. If the higher class of workers in Ireland took the trouble to study systematically the objects here so carefully described, an epoch might be marked in the development of Irish technical skill.

LETTERS TO THE EDITOR.

[The Editor does not hold himself responsible for opinions expressed by his correspondents. Neither can he undertake to return, or to correspond with the writers of, rejected manuscripts. No notice is taken of anonymous communications.]

[The Editor urgently requests correspondents to keep their letters as short as possible. The pressure on his space is so great that it is impossible otherwise to insure the appearance even of communications containing interesting and novel facts.]

The Duke of Argyll's Charges against Men of Science.

THE Duke of Argyll's singular appetite for besmirching the characters of men of science appears to grow by what it feeds on; and, as fast as old misrepresentations are refuted, new ones are evolved out of the inexhaustible inaccuracy of his Grace's imagination.

In the last two letters which the Duke of Argyll has addressed to you, he accuses me of having charged the members of the French Institute with having entered into a "conspiracy of silence" in respect of Mr. Darwin's views. I desire to say that the assertion that I have done anything of the kind is untrue and devoid of foundation.

My words, in the passage of which the Duke has cited as much as suited his purpose, stand as follows: "In France, the influence of Elie de Beaumont and of Flourens—the former of whom is said to have 'damned himself to everlasting fame' by inventing the nickname of 'la science moussante' for evolutionism—to say nothing of the ill-will of other powerful members of the Institute, produced, for a long time, the effect of a conspiracy of silence."¹ I used the words I have italicized advisedly, for the purpose of indicating that, though the members of the Institute did not enter into a conspiracy of silence, the notorious antagonism of some of them to evolution produced much the same result as if they had done so.

If the Duke of Argyll were properly informed upon the topics about which he ventures to speak so rashly, he would know that M. Flourens wrote a book in vehement denunciation of evolutionism. As I reviewed that book not very long after its appearance, I could not well be ignorant of its existence. And being aware of its existence, I could not possibly have charged M. Flourens with taking any part in a "conspiracy of silence."

The "effect" of the known repugnance to Mr. Darwin's views of some of the most prominent members of the Institute, to which I refer, is the effect upon the younger generation of French naturalists. Considering the influence of the Institute upon scientific appointments, the chances of a candidate known to be an evolutionist would have been small indeed; and prudence dictated silence.

Mr. Carlyle has celebrated the courage, if not the discretion, of a certain "Rex Sigismundus," who, his Latin being called in question, declared that he was, as a Royal personage, "supra grammaticam." The Duke of Argyll appears to be of King Sigismund's opinion in respect of the obligations which are felt by humbler persons, who have, wittingly or unwittingly, accused their fellows wrongfully; and I do not suppose that he will descend, on my account, from a position which may be sublime or may be ridiculous, according to one's point of view. The readers of NATURE will choose their own.

T. H. HUXLEY.

Bournemouth, February 4.

¹ "Life and Letters of Charles Darwin," vol. ii. pp. 185-85.

An Explanation.

SINCE the Duke of Argyll's references to myself have been interpreted in a manner likely to convey an erroneous impression to the readers of NATURE, it seems to me to be now necessary to give some explanation of the facts in which I am concerned. I intend, however, to go no further than to establish the position his Grace has taken up as regards myself. Such a step, savouring somewhat of presumption on my part, would not have been taken if Prof. Judd had admitted that, although no paper of mine was ever before the Council of the Geological Society, an offer to present such a paper was, doubtless for sufficient reasons, at once declined.

In the spring of 1885, by the advice of Mr. Murray, who had been for some time engaged in examining my recent geological collections from the Solomon Islands, I offered to Prof. Judd, then Secretary of the Geological Society, to present my observations on the upraised coral-reef formations in the form of a paper, in which, as I stated, Mr. Darwin's theory of coral reefs would be brought under consideration. This offer being declined, my observations were taken up by Mr. Murray and were published in the Transactions of the Royal Society of Edinburgh for 1885. As I saw too plainly that the new view of the origin of coral reefs was very far from being generally accepted, I deemed it advisable in preparing my paper to draw no inferences and to allow the facts to speak for themselves. However, six months after the reading of the paper, whilst going over the proofs, having been assured that the theory of Mr. Darwin was rapidly losing ground, I appended some remarks in which I gave the general bearing of my discoveries.

Had I harboured a desire in my mind to record any disappointment in connection with the appreciation of my work, I might have done so in the preface of my small geological volume recently published. The reflection that I had succeeded, and that Mr. Murray's views, as I was told, were being generally received, gave me ample grounds for satisfaction; and there was therefore no reason why I should refer to any difficulties of a personal character. I must confess, however, I was afterwards deeply disappointed on finding that, although the nature of my discoveries was first announced in the columns of this journal in January 1884, whilst the observations themselves had been nearly two years before the world, my name and work were studiously ignored in the recent controversy by those who spoke on behalf of English men of science, and particularly on behalf of the Geological Society. Naturally it was there that I looked most for approval. I soon perceived, however, that it could not be in the want of publicity that the reason lay, nor even in the insufficient lapse of time since the publication of my papers. Long abstracts were given in the columns of this journal of the principal paper (Trans. Ed. Roy. Soc., 1885), and of a paper also read before the Royal Society of Edinburgh (Proc., 1886). At the beginning of 1885 (or perhaps earlier) I sent to Prof. Judd a blue pamphlet published in New Zealand, in which I briefly described the discoveries I had made up to the end of 1883. At the beginning of 1886 I sent to him my principal Edinburgh paper of the previous year.

It then occurred to me that since Prof. Dana's last paper, of September 1885, was the chief rallying point of the opponents of Mr. Murray's views, the cue in estimating the value of my work might have been thence derived. I found, however, that Prof. Dana had only before him, when referring to my discoveries, an extract from a private letter of mine to Mr. Murray written in the midst of my work, and published in NATURE in January 1884. Rightly enough, he did not consider such a brief account as at all conclusive. My published observations had yet to come before him. It was not, therefore, from the other side of the Atlantic that in estimating the value of my observations Mr. Murray's opponents had taken their cue.

I was forced, therefore, to the conclusion that the reason lay rather in the competency than in the bearing of my observations. I could find no other explanation of the fact that in the succession of replies to the Duke of Argyll's article, entitled "A Great Lesson," no reference whatever was made to the recent important evidence I had adduced—evidence of which at least one of the writers had been previously aware during a period of two if not three years. Under these circumstances, I accepted the decision which the lapse of nearly three years had not affected; and, having naturally some degree of sensitiveness, I withdrew from the Geological Society.¹

¹ Mr. Guppy was induced afterwards to withdraw his resignation.—ED. NATURE.